informative synopsis in one or more world languages. In time, perhaps, the *Interlingua* will serve for this purpose as the universal language of scientific summaries and will thus restore, to some extent, the intellectual unity of the Western world, lost with the replacement of Latin by national tongues. In smaller countries, still wishing to be heard in the international forum, a program for providing technical help in the preparation of idiomatically correct synopses and abstracts of scientific publications would fully deserve the attention of the local academies of science.

There is another important step that the author (and the publisher) can take in the interest of making a scientific communication more widely and more thoroughly intelligible—namely, presentation of legends, table headings, and graph labels in a language of world-wide currency. In particular, we have stressed this point for years to our Japanese colleagues and friends.

It has been a real pleasure to see in a recent paper on the anthropological differences between population groups in Czechoslovakia [V. Fetter, *Ceskoslov. Ethnograf.* 5, 217 (1957)] the consistent use of both Russian and English, in addition to Czech, in tables and graphs. In sciences which operate with quantitative data, the tables and graphs contain, as a rule, the essential new information. Together with a clear verbal summary of conditions and methods, the presentation of tabular and graphic material in a form intelligible to the majority of scientists will go a long way toward removing the curse of nationalism and provincialism in scientific publications.

In works which rely heavily on pictorial documentation, such as Hess' atlas [W. R. Hess, *Hypothalamus und Thalamus* (Thieme, Stuttgart, 1956)], the use of bilingual legends may be both preferable on purely scientific grounds and more economical in the long run.

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"Psychozoa"

In the note by William L. Straus, Jr., on "Evolutionary terminology" [*Science* 127, 22 (3 Jan. 1958)], there unfortunately occurs a protest about Huxley's use of the term *Psychozoa* for man, presumably on the ground that our social world's state today does not indicate anagenesis with reference to man, but the opposite. If, by what Straus terms the "increasing disintegration of human interpersonal and intersocial relations," he refers to the radical alteration of social affairs in our time, this might very well be cited as evidence of the adaptability of the type and, possibly, as leading to its "biological improvement." The "disintegration" of a social order is not necessarily to be interpreted as evidence for nonanagenesis in man, nor is it to be considered as sufficient grounds for withholding the accolade—if such it is—of "Psychozoa" from man.

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Dr. Chester W. Hartwig seems to have misinterpreted the reason for my protest against Julian Huxley's coinage of the taxonomic category "Psychozoa" for man. His misinterpretation can perhaps be traced, at least in part, to an unfortunate misprint by which my original "intersocietal" was altered to "intersocial." I thus was not referring to "the radical alteration of social affairs in our time" but, rather, to the frightening disintegration of relations between nations and international coalitions, a disintegration that actually threatens the very existence of the human species. The evolution of man until 1914 may well have been one of increasing adaptedness and so-called biological improvement—hence, "anagenetic"; but one may justifiably wonder whether it has not been going in the opposite direction since that time. Indeed, if one were inclined to coin a bit of the evolutionary jargon against which I protest, he might will be tempted to label man's recent evolution "katagenetic."

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Worms, Dogs, and Paramecia

The current debate in *Science* between Gelber (*1*) and Jensen (*2, 3*) concerning the learning ability of paramecia focuses attention upon a new version of an old argument. In a series of experiments begun some ten years ago, Gelber (*4*) has sought to discover whether *Paramecia aurelia* can be conditioned. She employed a typical conditioned-response paradigm. Following the replication of some of her studies, Jensen has maintained that the data do not demonstrate learning but are better accounted for by chemical processes. To clarify her findings, Gelber (*1*) has drawn the analogy between a hungry dog approaching a steak and paramecia swimming toward food-bacteria. A more apt comparison, thinks Jensen (*3*), would be "that of an earthworm which crawls and eats its way through the earth, blundering onto food-rich soil and avoiding light, heat, and dryness." The situation has its humorous aspects.

The basic question at issue may be stated in more general terms in the form of two conflicting hypotheses. (i) The hypothesis espoused by Gelber is that the ability to learn or to modify behavior with practice is a function of all living tissue. It should therefore be demonstrable not only in paramecia but in other low organisms as well. (ii) The hypothesis espoused by Jensen is that learning ability is possessed only by animals relatively high in the phyletic scale and that the behavior of lower forms must be accounted for in terms of reflexes, tropisms, and so on—that is, mechanically. Comparative psychologists and students of animal behavior will recognize in these two viewpoints a dichotomy which has existed in one form or another for centuries. Sometimes it has been given a religious flavor, the line being drawn between man, who possesses reason, a soul, and other high powers, and animals below man, which are lacking in one or more of these attributes. Such great names as those of Aristotle, Thomas Aquinas, Descartes, and Jasques Loeb have been associated with this problem.

Perhaps more to the point in the present controversy is the citation of relevant literature on the learning (and nonlearning) of lower organisms. It appears not to have occurred either to Gelber or to Jensen that they might have bolstered their cases in this way. Day and Bentley (*5*), for example, have reported that they got learninglike behavior in paramecia in a situation which was entirely different from that of the Gelber experiment. The Day and Bentley study was duplicated by Stevenson Smith (*6*), who supported the major findings in almost every detail. Learned avoidance responses to heat, cold, and ultraviolet light have been observed independently by Bramstedt (*7*), Soest (*8*), and Tschakhotine (*9*). A different type of investigation, by French (*10*), gives seemingly unequivocal evidence of trial-and-error learning in paramecia. Appropriate controls ruled out any question that the change in behavior with repeated trials might have been due to chemical effects or to fatigue.

Reported examples of learned behavior in other primitive organisms also bear upon the argument. According to Warden, Warner, and Jenkins (*11*), the common marigold, which opens to light and closes to darkness, can be "trained" to a particular rhythm of opening and closing by repeated exposure to alternating light and dark periods of the desired frequency. Gibbs and Dellinger (*12*) noted activity in *Amoeba proteus* which they considered to be learned, and Mast and Pusch (*13*) succeeded in training *Amoeba* to make an anticipatory avoidance response to a beam of light. In describing the research of N. N. Plaviltschikob on the conditioning of 82 colonies of infusoria, *Carchesii lachmani*, Razran (*14*) has referred to this work as "the most extensive single experiment in the conditioning of any organism."

These studies constitute a considerable